

I spoke of heat passing across a stratum of gas from one solid surface to another "as though there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as [it would perhaps have been better to have said "determined by"] that of the contiguous solid."

I am fully aware of the ease with which one may be led into serious mistakes by trusting too implicitly to such simplifying assumptions, and also that some of the particular suppositions made above would be inadmissible in a discussion of the general problem of the conduction of heat in gases; but I do not see any fallacy in employing them for the special purpose which I had in view in my last letter, namely, to show why I think that the flow of heat across a thin stratum of gas must be facilitated by diminishing the pressure of the gas. Prof. Osborne Reynolds's argument that "if there were a layer of uniform temperature, no heat would be transmitted," does not appear to me to be applicable to the case in question. It seems conceivable, as an extreme case, that, in a very thin layer of gas, between parallel solid surfaces maintained at different temperatures, the molecular movements might take place exclusively in the direction of the perpendicular to the bounding surfaces. In such a case the particles would move from side to side of the layer of gas with a uniform velocity, though the velocity one way would be greater than the velocity the other way, and heat would be transmitted across a layer of gas having the same temperature throughout. Such a condition, whether practically realisable or not, would, if I understand him aright, be the limiting case in one direction of what Mr. Stoney has called for shortness a "Crookes's layer:" the limiting case in the other direction being the ordinary condition of a gas, where the average velocity of the molecules is independent of direction. I venture to think that, in pointing out the results which must follow from the existence of a predominating direction of molecular motion, Mr. Stoney has made a very important contribution to the kinetic theory of gases; and I do not see that his conclusions are in any way invalidated by its being shown that they are not in harmony with "the generally-accepted laws of gases," inasmuch as these laws are deduced from suppositions which expressly exclude the conditions he has investigated. G. CAREY FOSTER

December 17

ALLOW me to say a few words on what I believe to be the correct theory of the radiometer. This theory was given to me by Prof. Osborne Reynolds during spring of 1875, and I have found it capable of explaining every experiment on the subject with which I am acquainted.

The conservation of momentum is one of the laws of nature which even molecules do not break, and that law puts some restraints on the wonderful things which the shocks of molecules can accomplish. Imagine a vessel full of gas at a certain temperature. The centre of gravity of the gas and that of the vessel are supposed to be at relative rest. Suppose now that I increase the velocity of a certain number of molecules in a given direction, the centre of gravity of the gas will move relatively to the centre of gravity of the vessel, and no number of encounters between the molecules can alter that motion until the momentum has been taken up by the vessel. If in any gas we have a passage of heat in a certain direction, we shall have a propagation of momentum owing to the fact that the molecules move more quickly in one direction than in the opposite one, and no number of encounters can alter that propagation. Where the momentum enters the gas and where it leaves it we observe certain forces. This is Prof. Reynolds's theory of the radiometer. It has been objected that an increased pressure on the cool side of the vanes of a radiometer will counterbalance the force acting on the blackened sides, when the dimensions of the vessel are large compared with the mean path of a molecule, but I do not think that such is the case. The following special case may make this point a little clearer. If the forces on the vanes are counterbalanced, the forces on the vessel must be counterbalanced as well. In the case of an ordinary radiometer the forces reduce to a couple, and I do not see how any crowding of molecules in one part of the vessel more than in another can produce a couple on the vessel. The whole problem is one of conduction of heat. All the experiments made by Mr. Crookes on cups, inclined vanes, &c., admit of the same easy explanation as the fact that when a long and a short wire are connected with the poles of a battery, the current in the shorter wire will be the strongest. In a radiometer with inclined vanes, for instance, the

temperature is the same on both sides, but the gradient of temperature is much larger on one side, and hence more heat will escape on that side. The dimensions of the vessel also have to be taken into account in the same way as the length of a wire has to be taken into account when the strength of an electric current flowing through it has to be calculated. It is difficult to say exactly what takes place within very small distances from the hot surface, but it seems clear that any phenomenon, such as Prof. Carey Foster supposes to exist, must affect the passage of heat in the same way as the force on the vanes. As the careful researches of Messrs. Kundt and Warburg have shown that under great exhaustion the conduction of heat decreases and does not increase, I do not see how an increase in the force can take place.

The scientific world will judge how far Prof. Stoney has succeeded in establishing any new laws on the conduction of heat through gases. In justice, however, to Messrs. Provostaye and Dessains, whose experiments he calls to his aid, I wish to point out that their numerous experiments, with two exceptions, are in entire accordance with existing theories. At the time these experiments were made, no distinction was drawn between convection and true conduction. In order to deduce, therefore, the loss of heat due to true conduction, Prof. Stoney is obliged to subtract the effect due to convection currents. He draws, therefore, a curve representing the loss of heat due to this cause. All his conclusions must stand or fall with this curve, and I am afraid they must fall.

After Professors Clausius and Maxwell had deduced theoretically the coefficient of conductivity for gases, a series of celebrated experiments were made by Stefan, by Narr, by Plank, by Winkelmann, and last, but not least, by Kundt and Warburg. The influence of convection currents has been fully discussed in these papers and eliminated, and the conclusions arrived at by all these experimenters are fully in accordance with each other and with theory. It appears, as was expected, that when the effects of convection currents are eliminated, the coefficient of conductivity is independent of pressure until the dimensions of the vessel are comparable with the mean free path of a molecule, and that then the conductivity rapidly diminishes. It also appears that at the pressures at which Messrs. Provostaye and Dessains found that the loss of heat was independent of pressure, convection currents must have ceased to be appreciable, and therefore the great mass of their experiments is fully in agreement with later researches.

The only exception is found in the case of carbonic acid and nitrous oxide. These abnormal results were not confirmed by Messrs. Kundt and Warburg in the case of carbonic acid, the only one of the two gases which they examined. Whoever reads their account of the difficulty they had in excluding the last traces of moisture, and considers the increased conductivity which such an admixture would produce as the pressure diminishes, will have no difficulty in explaining the anomaly. At any rate I do not think Prof. Stoney will be inclined to base important conclusions on unconfirmed experiments on two gases in which we should expect the effect, owing to their density, to be particularly small. The discovery of Master Gerald Stoney, who found that a red hot wire was cooled when a tin can containing water was brought sufficiently close might, I think, have been foretold by the recognised theory. Prof. Stoney, no doubt, will find on reading over the literature on the subject, that what he calls penetration of heat, has hitherto been known under the name of conduction of heat, that it takes place at all pressures, and begins to disappear at the exact point at which he makes it appear.

The timely calculation of Mr. S. T. Preston in the August number of the *Phil. Mag.*, shows that any theory of the radiometer which makes the action depend on the comparatively large ratio of the mean free path to the dimensions of the vessel, must necessarily be wrong. ARTHUR SCHUSTER

The Proposed Channel Islands' Zoological Station, Aquarium, and Piscicultural Institute

I AM very anxious that this project should succeed, mainly because of the facilities it will afford to inland aquaria, in procuring living animals cheaper, better, more variedly, and more systematically, than at present. This, I believe, will form the most profitable part of the undertaking.

* Referred to in NATURE, vol. xvii. p. 102.

I hope, also, it may succeed as a sort of living museum, without any of the kind of attractions which are not biological ones, and which, indeed, are not scientific in any sense, as common and ribald music, theatricals, acrobatic and jugglery performances, and so forth. Only, no aquarium has ever permanently thrived without these things.

I greatly doubt whether anyone yet possesses the requisite knowledge to be able to rear any marine crustacean from the egg state to an adult condition, and to feed it in captivity in such manner as to be able to sell it in the open market at rates below those sold under present circumstances. Yet this is put forward, conspicuously, as one of the aims of the scheme. In Britain are eaten as human food about a dozen species of crabs, lobsters, prawns, and shrimps, and most of these have been occasionally bred in aquaria as far as the Zœa state, when they are free swimmers, and they then generally die. Rarely, some few have been brought up to a higher stage, but I know of no instance, during twenty-five years of experience, where any marine crustacean of any kind has been reared to an adult condition in an aquarium. And if such a thing could be done, I believe that to feed them in any state of captivity, with animal food, which they require in great abundance and variety, and which must be purchased, would be very expensive, indeed, far too costly I think, to be practically and commercially remunerative.

I am glad to see that in last week's NATURE, vol. xvii. p. 133, it is stated that that excellently-tasted little fresh-water lobster, *Astacus*, has been bred artificially by a Piscicultural Institution at Schwerin. If so, why should it not be similarly bred in Britain, where it is much seldomer eaten than in France and Germany. In Berlin, Hamburg, and Dresden, I have often purchased it at sixpence a dozen, while in Paris I have given as much as sixpence each for it. It is a pity, however, that the Schwerin account is not more full and explicit. It is stated that in the spring of 1876, 700 *Astacus* in egg, were placed in two round ponds, each of six feet diameter. Holes were made in these, and recently, on draining the ponds, only three or four adult crayfish were found straying about the ponds, the rest each being in a separate hole, and a large number of young ones were found, as big as bees, and very lively. What size were the crayfish at birth, and if very small, and swimming, how were they prevented from escaping from the ponds? Information is wanted as to the shape, length, breadth, direction as to angle, and distance apart of these holes, and their position in the ponds, whether in the sides or base, or both. If nearly 700 animals occupied as many holes, where were the young ones? How many young were there? If each female had only as few as 100 eggs hatched out, then 70,700 must have been the population of these two little pools. When, and in what manner, were the males introduced? We require also to be told of the material of which the ponds were constructed, and if the sides were upright, and the bottoms flat, or if rounded or basin-shaped. If water ran in and out, how much in a named time, of what quality, as to foreign substances it contained in solution and suspension, and what was its temperature at various periods of the year? In what direction and in what amount was light admitted? How much vegetation, and of what kinds, grew in the ponds? What kinds of animal food was given them, and how much and often, and was it cooked or raw? Carrots appear an odd food for crayfish. Let all these things and more, be carefully ascertained, to see if they can be applied to the culture of *Homarus*, the near marine relative of *Astacus*, before much money is spent.

It would be an excellent thing for students to have a place to study at, such as is proposed to be provided for them at Jersey, and similar to the zoological station and aquarium at Naples, in the arrangement of which I had much to do. But would students be content to go only so far as Jersey? Is not the access too easy, and too cheap, as it is not easy or cheap to go so far as Naples, and to have the name of so going? I have often thought it odd, and evincing not at all a really zealous spirit in my own direction on the part of my fellow-naturalists, that such a thing should be, that though the Crystal Palace Aquarium has existed for seven years within less than one hour's railway ride from London, and though it contains a constantly advertised collection of living marine animals exceeded in variety and interest by none in Britain, or even Europe, yet no scientific man, except the late Edward Newman, has ever applied for permission to carry on any course of inquiry here on any subject, continuously or occasionally, connected with the habits of living creatures, in the spirit of say, Gilbert White of Selborne. Yet we offer

all such advantages as table-space, good light, and the use of any animals in our tanks not having a considerable money value—lest injury be done to such specimens—absolutely free of all charges. At this moment we possess many Italian animals in our collection, as fishes, crustaceans, mollusks, zoophytes, &c., which can be seen alive nowhere else, save on the shores of the Mediterranean, and yet no professed zoologist known to me ever comes to see them, or takes the smallest interest in them. They are therefore beheld only by the general public, who only look at them for their mere prettiness, or for what untrained observers are complacently pleased to term "ugliness." No student ever asks us for more than any dead animals we may chance to have, and which we give away gratis, and these apparently afford far more pleasure than the sight of living specimens. It is not at all uncommon to meet with biologists who openly and avowedly proclaim their contempt for collections of living animals in aquaria, which they regard as being "well enough for women and children," but for men they say there is nothing like seeing such animals in rows of glass jars of alcohol on the shelves of a museum. My last contribution of any length to NATURE was made so long ago as October 12, 1871, when I gave a description of the Crystal Palace Aquarium, then only just opened. Since then we have had no cause to complain of the appreciation of the world of sightseers. But as regards the indifference of the scientific world, that has been and is so great that the place might never have existed. William Yarrell, the British ichthyologist of the generation just passed away, used to tell me how glad he would be to see a live John Dory (*Zeus faber*), and how much he would give to behold one swimming. But here, at Sydenham, this fish can be seen alive and in perfect health for months together, in crystalline sea-water. Yet no Yarrell ever comes to see them. Are there no Yarrells, and Whites, and Watertons, and Newmans now? or has their very spirit passed away into the region of apathy where the affectation of caring for nothing, and of being never moved to zeal in anything, in this observation of live animals, seems to be regarded as a very high accomplishment?

W. A. LLOYD

Crystal Palace Aquarium, December 15

The "Challenger" Estimates of the Volume of the Gulf Stream

In the interesting "Voyage of the *Challenger*," just published, Sir Wyville Thomson states (vol. i. p. 371) "that the Gulf Stream in its restricted sense was, early in May, 1873, at the point where we crossed it and made our observations, about sixty miles in width, 100 fathoms deep, and its rate three knots an hour." I was much surprised at reading this, as the Admiralty Report on Ocean Soundings, No. 7, p. 12, estimates it at the same point as "100 fathoms deep, and running at the rate of three miles an hour for a width of fifteen miles, discharging four and a half cubic miles of heated water per hour."

As no reference is made by Sir Wyville Thomson to the extraordinary discrepancy in these two estimates of the same thing at the same time—one being four times the volume of the other—and as he says he makes the statement "thus guardedly" I think, in the interest of scientific accuracy, an explanation is required.

T. MELLARD READE

Liverpool, December 8

The Fossil Peronospora as a Primordial Plant

THE concluding sentence of your notice (vol. xvii. p. 128) of my observations on a fossil fungus is, so important, that I shall be glad of a word of reply. You say, "But should not this primordial plant have led a non-parasitic life? for if parasitical, then this fact points to some pre-existing plant."

Although the specimen I have figured is shown as growing within the decayed tissues of a *Lepidodendron*, yet it does not follow that the same fungus could not perfect itself on humus alone. Recent species of *Peronospora* show a tendency to grow upon the ground, as several species, including the fungus of the potato disease, will grow and produce fruit on the naked earth. A truly terrestrial species is found in the allied *Botrytis terrestris*, Persoon, and many of the *Mucedines* grow freely in cellars, on damp walls, or in any moist place.

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